Author's response to reviews

Title: All-cause and Cardiovascular mortality among ethnic German immigrants from the Former Soviet Union: a cohort study

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Author’s response to reviews: see over
Dear editor,

we thank you for having considered our paper for peer review. We truly appreciate the constructive and helpful comments by the reviewers. We have thoroughly revised our manuscript according to their suggestions. Please find attached a detailed response to each of the comments.

We hope that with our revisions we were able to successfully address all concerns the reviewers had regarding the manuscript and that the paper can now be accepted for publication. Once again, we would like to thank you very much for your efforts.

Yours sincerely,

Ulrich Ronellenfitsch
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Answers to reviewers

Reviewer 1

Discretionary revisions

The authors expected to find a higher mortality among the Aussiedler, but found the opposite, why it is important to search for possible sources of error in the study, e.g., selection bias, drop-outs etc, and confounding factors. However, the authors discuss possible factors, and it seems very unlikely that these factors could have other than a marginal influence on the results. Well then, are the findings realistic? The authors state as regards the Aussiedler, that “their overall mortality in the FSU would need to be only 40% and their CVD mortality only a third of the average in these countries, which seems improbable”. Well is it really improbable, when considering this magnitude of difference? In most countries women show a mortality rate of 50% of men in most ages, corresponding to a higher mean life expectancy of 4-5 years. We also know that there are large differences between different countries, as well as between different groups in the same country. The INTERHEART study found a strong association between psychosocial factors and acute myocardial infarction, one of the most important diseases attributing to both CVD and total mortality (Rosengren A, et al. Association of psychosocial risk factors with risk of acute myocardial infarction in 11 119 cases and 13 648 controls from 52 countries (the INTERHEART study): case-control study. Lancet 2004; 364: 953-62). The psychosocial factors don’t only conclude socio-economic status, but also other factors, which could be hard to measure in register-data. Taking some examples, when looking at neighbourhood areas in Chicago, life expectancy for men ranged from 54 to 77 years (ref Wilson M, Daly M. Life expectancy, economic inequality, homicide, and reproductive timing in Chicago neighbourhoods. BMJ 1997: 314:1271-4). Another example is that the CHD mortality among in 50-year-old Lithuanian men is four times higher than among 50-year-old Swedish men, despite similar presence of standard risk factors, probably due to psychosocial risk factors, as concluded by the authors (ref Kristenson M, Kucinskiene Z, Bergdahl B, Calkauskas H, Urmonas V, Orth-Gomer K. Increased psychosocial strain in Lithuanian versus Swedish men: the LiVicordia study. Psychosom Med 1998; 60: 277-82).

As regards ethnic groups and differences in mortality: research from Finland has shown, that the mortality among members of the Swedish-speaking minority is lower compared to the Finnish-speaking majority (Hyyppa MT, Maki J. Social participation and health in a community rich in
It was concluded, that a great deal of health inequality seems to derive from uneven distribution of social capital. Perhaps this could also be the case as regards the ethnic Germans in the FSU. At least it is a hypothesis that could be considered and analysed. Some findings in the study, i.e. a lower mortality among the older immigrants and those belonging to a larger family group, could support this hypothesis. I think the Discussion in the article could be more informative and interesting when considering these factors that are mentioned.

We thank the reviewer for pointing out that large disparities in mortality are not an uncommon phenomenon in Public Health, and that even the putative 60-70% difference in mortality between the Aussiedler in their countries of origin and the general population in the FSU does not have to be absolutely unrealistic. We gratefully included the provided references into our text and added more information on pertinent mortality disparities between different subgroups of populations [discussion section, paragraph 2]. We fully agree that psychosocial factors most probably are important determinants of migrants' mortality and play a crucial role in our study population. We now elaborate on that more extensively when talking about the socio-economic status of Aussiedler in Germany [discussion section, paragraph 3] by pointing out that "social cohesion" might be highly important. We purposely avoid to talk about "social capital" because using this term would require a definition and background information in which context it was developed, which would in our opinion be beyond the scope of this paper. We are also not sure if it could be correctly applied to our study population without misleading the reader - the term "social capital" as defined by Putnam implies strong civic organisations and networks of associations for the existence of which among the Aussiedler we don't have any evidence.
Reviewer 2

**Major compulsory revisions**

**Background**

1. More background literature on the phenomenon of “healthy immigrants” would be helpful, as would more information regarding differences in health status and mortality rates for immigrants in relation to those of their new host countries. Examples from the literature might include other studies that have found that immigrants had lower rates of CVD than native-born US citizens (Singh & Siahpush, 2002), but immigrants to Sweden had a higher CVD risk than the general population (Dotevall et al., 2000). See also Nair et al., 1990 regarding immigrants to Canada. Mexican immigrants to the US tend to have better health than longer term or second generation immigrants. Including studies on other ethnic groups would help place this one in a more global context. If these studies and others like them are reviewed in the background, the findings from this study can be compared to them in the Discussion section. Also, comparing methods for data analysis with these studies might be helpful.

We agree that the rationale for the conduction of our study was not sufficiently pointed out in the introduction. Therefore we have rewritten the part of the introduction which leads to our study hypotheses [background section, paragraph 3]. It now contains more background literature on mortality patterns observed in migrants, including some of the references suggested by the reviewer and other important papers in the field such as the review by Kliwer et al. This should help to put our study in a more global context and makes the deduction of our hypotheses more comprehensible. As suggested, we now refer to the presented studies again in the discussion and compare our results with their findings [discussion section, paragraph 2]. We decided against providing in an extensive comparison between the methods applied in our and the cited studies. We think that this would be beyond the scope of this paper and make it tedious to read. We do, however, inform the reader about the strengths and weaknesses of our study design and data collection and point out possible sources of bias and how these could potentially affect our results [discussion section, last paragraph].

2. Some information regarding the social status or identity of the ethnic Germans in Russian society would be useful. For example, did they tend to be different in any way from the mainstream of Russians? Socialize together? Have higher education or job status? Maintain religious or cultural affiliations? The purpose of this would be to see if there might be sociocultural reasons for their lower CVD risk. Russians who maintained their ethnic Jewish identity and Russians who were Baptists, for example, are known to have had much lower rates of alcohol use than the general population. Also, even if the ethnic German immigrants have lower SES in Germany as noted in the manuscript, they may have had a higher SES in Russia, and the effect of SES is probably longer term than the period of time in this study.

As suggested by the reviewer, we have extended the information given in the background section on the socio-economic status of Aussiedler in the FSU as compared to the general population of these countries, and we elaborate on potential differences [background section, paragraph 5]. In the discussion [discussion section, paragraph 2] we refer to these differences and point out that cultural particularities might play a role in the low mortality observed among Aussiedler. We support this view with findings among Russian Jews and Jewish migrants from the FSU to Israel.

3. The authors have not made a clear case for their hypothesis in the background. Perhaps they should include primarily the data supporting the hypothesis that immigrants would have higher CVD mortality rates to show why they originally thought that argument was more compelling. There is some data in the literature that the stress of migration per se leads to higher rates of CVD morbidity;
this could support the study hypothesis. Then alternative explanations can be discussed in more
detail in the discussion section.

We disagree with the reviewer’s opinion that we have not sufficiently made clear how our hypothesis that Aussiedler have a higher mortality has evolved and that we have not provided data supporting it. We present figures for mortality rates for CVD in the FSU and Germany, and we have now also added the respective figures for overall mortality [background section, paragraph 4]. It is somewhat unclear what the reviewer means with “the stress of migration per se”. We agree that migrant status as such frequently leads to multifaceted socioeconomic disadvantages in the host country in terms of limited economic opportunities but also of socio-cultural isolation. We have added a sentence to make it clearer that we account for that fact in the second group of determinants of migrants’ mortality in the host country. We believe that by thoroughly looking at these three groups of factors in the context of Aussiedler from the FSU, which is exactly what we did in the background section, our hypothesis becomes a logical consequence.

Methods

1. More detail in the methods section would help the reader understand what was done in the study. “Vital status” is an unfamiliar term to me. Also, more information on the way the German government assigns immigrants to states and cities would be of interest to readers from other countries who are not familiar with this process.

The reason why initially we did not include a detailed definition of the term „vital status“ is that it is commonly used in epidemiology. We thank the reviewer for the reminder that not all readers are familiar with the epidemiological language and have added an explanation of the term [methods section, paragraph 2]. We also included more information on how the migrants are assigned to the single federal states and municipalities and why we consider our selection of participants leading to a random sample of all Aussiedler coming to Germany during the study period [methods section, paragraph 1].

2. The reason for stratification by family size is not obvious—is there any literature to support doing this?

As requested, we provided some references which support our rationale of family size stratification [methods section, paragraph 1].

3. It is not clear from the beginning what age of the German population was used and how this was comparable to the immigrants. Perhaps looking at immigrants from age 15 is too early for differences to be apparent?

We added an explanation specifying that in the SMR analysis we compared mortality within identical five-year age strata in our cohort and the German population [methods section, paragraph 4]. This implies that also the data on the German population only comprises figures from persons being 15 years of age or older. We do not agree that including people from age 15 onwards in our cohort blurred any possible mortality differences. Actually, we performed separate analyses for different age groups and were thus able to reveal the specific age-dependent mortality pattern of Aussiedler per se and relative to the German population. This is even appreciated by the reviewer in her general remarks on the paper. Furthermore, limiting our analysis to participants above a certain, higher, age, would not have added any statistical power to the study since this depends directly on the number of observed deaths in the cohort.
4. The use of the Poisson regression analyses is not clearly explained. I am not familiar with it, and the results do not seem to be presented in the way multivariate regressions are usually done in other journals. The units of analysis are not spelled out and more detail on the statistical methods would be helpful.

We are aware that Poisson regression is not a straightforward statistical technique. Nevertheless, we are convinced that from the epidemiological point of view it is the most appropriate statistics for the specific purpose of our study. To fully explain such a complex method in a paper reporting original research is virtually impossible without breaking any word limit. This is why the description of the technique was rather concise in the first version. We have now added some more detailed description [methods section, paragraph 5], but we are still aware that someone who is unfamiliar with the method will most probably not be able to fully understand how it works from our paper. This is why we have provided a reference (Breslow & Day 1987) which extensively describes Poisson regression. Furthermore, already in the results section [paragraph 1 of subsection ‘Poisson regression’] of the first version of our paper we had purposely included a short “instruction” on how to read and interpret the results in tables 4 and 5. We have tried to refine this “instruction” in order to make the tables even more comprehensible.

Discussion

1. The discussion, as noted above, would benefit from somewhat different organization. Perhaps the authors could start by comparing their findings with other studies in the literature of the same or different population, and then suggest explanations for the findings of this study specific to this sample.

As suggested, we have somewhat restructured the discussion. After giving a short summary of our most important findings, we start by comparing our results to those from studies on migrants in different countries which were already mentioned in the introduction [discussion section, paragraph 2]. Unfortunately we are not able to compare our findings to those from other studies on the mortality of Aussiedler because our study is the first to look into that topic.

2. In the discussion, the authors should talk about why the transition to living in Germany might or might not be particularly stressful for this population. In general, more literature regarding other ethnic groups would be helpful to round out the background rather than limit the review of literature only to immigrants to Germany.

We agree with the reviewer that the capacity to deal with the transition from living in one country to settling in another one is a determinant of mortality. We had already argued so in the original version of the paper, but maybe this did not become sufficiently clear. Therefore, we have tried to stress this notion by suggesting some more detailed mechanisms how such an association could look like. We focus on socio-economic status and the formation of an own cultural identity [discussion section, paragraph 3] as well as the putative beneficial effect of large family size [discussion section, paragraph 7]. Since our study was unfortunately not able to provide detailed original data on socio-economic determinants of health, we were rather careful in our explanations of possible relationships because without an empiric base everything that is postulated remains highly speculative. With regard to the suggested extension of the literature review, we have already pointed out above that we followed the suggestion and included further studies from several other countries.
Minor essential revisions

1. The use of the term “calendar period” to refer to the time of analysis is confusing and I wasn’t sure what it meant at first.

We have replaced the term “calendar period” by “study period” in the whole paper.

2. For the graphs, make it clear that the men are being compared to the male population; women to female population.

We have changed the legends of the figures accordingly, making it clear that men are compared to men and women to women.

3. The conclusion section could be clearer in its recommendations for future research and policy implications.

In the conclusions section, we have tried to clarify our recommendations for policy and research. In the light of the results that show that CVD mortality among Aussiedler shows no secular decline, which suggest shortcomings in CVD prevention, we recommend a thorough assessment and subsequent improvement of respective programs.

Discretionary revisions

1. There are a lot of similar tables; maybe could be decreased.

We are aware that our paper comprises a lot of tables and figures, but we hold the opinion that this is the most comprehensive way for reporting the wealth of different data it contains. Although they look alike, they all contain different information (SMRs and results from Poisson modelling for both overall and CVD mortality) of which we think that it cannot just be dropped. We also did not want to reduce the number of tables by putting too much information into the single tables because this would have made them very hard to read and understand. Therefore, we decided to leave the tables and figures as they were in the originally submitted version.